PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Polytobacco use and multiple-product smoking among a random community sample of African-American Adults
AUTHORS	Landrine, Hope; Corral, Irma; Simms, Denise; Bess, Jukelia

VERSION 1 - REVIEW

REVIEWER	Yerger, Valerie University of California San Francisco, Social and Behavioral Sciences
REVIEW RETURNED	16-Aug-2013

THE STUDY	One of the biggest shortcomings is the lack of detail applied to the study's community-based participatory approach (CBPR). The authors argue that telephone and household-interview surveys (randomized digital dialing survey methods) miss a large number of Black adult smokers, whereas the use of community-based sampling captures them. The authors should provide much more detail of their community-based sampling methods, which are innovative and can help inform how future sampling is conducted especially among marginalized groups. Other than a mere mention in the paper, the authors' community partners and their contributions to the study are absent. A community-based participatory approach implies community partners were instrumental in study design, data collection and interpretation of data. The authors reference Pichon LC, Corral I, Landrine H et al. Sun protection behaviors among African Americans. am J Prev Med. 2010; 38(3):288-295, which has a methods section that bears a close resemblance to the methods section in the manuscript under review. Yet the sample size is different, as is the amount of time it took the participants to complete the survey (15 min vs 20 min, respectively). The reviewer is confused. Were the two studies conducted at the same time? Were questions about sun protection asked at the same time as questions presented in this manuscript. Why the discrepancies in sample size and time to conduct the survey? The use of "non-smokers" is confusing. The authors do not adequately define what this means. What is even more confusing is the authors' use of "cigarette non-smokers." It's not clear the difference between a polytobacco product user and a multiple-product user? The CDC's MMWR Current tobacco use among middle and high school students - United States, 2011, August 10, 2012 / 61(31);581-585 is an important reference to cite. Though the findings presented in this report deal with youth, there are data to

	suggest that the use of non-cigarette tobacco products is on the rise among Black children. The data appear supportive of this study.
RESULTS & CONCLUSIONS	Given that 73% Black adult smokers smoke menthol cigarettes, the authors should discuss any potential implications of this heavy use of menthol cigarettes within the context of this study's findings regarding the other types of tobacco products. The Family Smoking Prevention and Tobacco Control Act of 2009 gave the US Food and Drug Administration the authority to ban the use of some flavored cigarettes, including clove cigarettes. Yet, the authors have not mentioned how this policy might or might not have an impact on the use of these particular products among Black adults. Otherwise, one might think that the study's findings regarding the kreteks/cloves no longer have relevance.

REVIEWER	**Allem, Jon-Patrick** University of Southern California, Preventive Medicine
REVIEW RETURNED	16-Aug-2013

THE STUDY	The authors' treatment of age is problematic e.g., producing inefficient estimates. In other words, arbitrarily turning a continuous measure into a categorical measure may produce bias estimates. Please see the following citation for a possible alternative approach: Allem JP, Ayers JW, Unger JB, Irvin VL, Hofstetter CR, Hovell MF. Smoking trajectories among Koreans in Seoul and California: exemplifying a common error in age parameterization. Asian Pacific J Cancer Prev. 2012;13(5):1851-6. PMID: 22901135 The above critique applies to income as well. It's not clear how or why these categories of income were selected. Perhaps the relationship with income and each outcome is curvilinear or exponential? Did the authors explore these possibilities? Moreover, did the authors explore possible interaction effects with gender and income or age? The gendered differences in outcomes among their participants suggest this might be a fruitful avenue to explore.
RESULTS & CONCLUSIONS	In the results section, the presentation and interpretation of estimates could be improved by discussing the estimated odds of each outcome given a characteristic controlling for covariates and by providing 95% confidence intervals in parentheses. The tables could be consolidated.
GENERAL COMMENTS	The authors presented a paper investigating the prevalence of cigars, bidis, kreteks, blunts, cigarillos and marijuana among a random, statewide sample of 2,118 California Black adult cigarette smokers and non-smokers. Overall, the study provided relevant findings regarding the prevalence, and correlates, of these distinct

	outcomes. I do have a small number of issues that should not be a major impediment to publication in BMJ Open. Major points: It's unclear why the authors employed blunts and marijuana use as outcomes in this manuscript. The polytobacco use literature the authors cited [references 1-4] in the introduction does not pertain to marijuana use by any vehicle e.g., blunts, joints, pipes etc. The authors should justify the inclusion of these outcomes in the introduction and subsequently highlight the clinical and/or policy significance of their findings in the discussion section (marijuana use was not discussed at all) or remove these outcomes from the manuscript. Perhaps these items would be more appropriate in a paper focusing on substance use. The authors stated, "However, most population studies of adults [1,3], unlike those of teens [4,8] did not assess smoking of bidis and kreteks," but failed to mention that these products have been banned in the U.S. since the passing of the Family Smoking Prevention and Tobacco Control Act. It would be informative if the authors demonstrated how participants acquired these tobacco products. If the authors did not obtain this information, they should mention how future research should be directed toward understanding how these products are sought out, and acquired, by consumers in the U.S., and how these practices could vary by race/ethnicity and how practices could be curtailed. It has been suggested elsewhere that the use of little cigars and standard cigars has increased over time due to increases in taxation and regulation of cigarettes. In other words, price sensitive consumers may alternate between little cigars and cigarettes. The dichotomous outcomes in the present study prevent the understanding of frequency of use of these products which limits the implications of the study. Could the authors detect if frequency of cigarette use is associated with the frequency of use of alternative tobacco products. This type of analysis may provide a clearer picture of how these products are used in relation to cigarettes. Additionally, the authors should note the relative price of these alternative tobacco products as a reason for their use and potential increases in use in the future. Minor points: In the introduction, the authors may want to structure hypotheses around their

	explanatory variables because their original hypothesis pertaining to “high prevalence” is ambiguous. The authors should report the year(s) data was collected. The authors may want to reorganize their discussion section so it concludes with a cohesive message about what should be done and where future research should be directed given their findings. This is in contrast to ending the paper with a list of limitations of the study.

VERSION 1 – AUTHOR RESPONSE

MS. YERGER'S COMMENTS

1. Ms. Yerger stated that the authors "should provide much more detail of their community-based sampling methods." The original manuscript explains in detail how we selected the counties, how we selected the census tracts within the counties, how we selected the block groups within the census tracts, and how we selected the homes within the block groups. We are unsure of what more is needed. Is there something specific about this sampling method that Ms. Yerger wishes us to add?

2. Ms. Yerger stated that we should say more about the role of the community partner in the study. We have added two paragraphs to page 6 of the revision to addresses this concern.

3. Ms. Yerger stated that our prior publication on sun-safety behaviors among this sample (i.e., Pichon et al 2010) had 69 fewer participants than reported here, and showed a slightly different time (20 minutes) to complete the survey. Dr. Pichon reported only on the number of participants who answered the sun-safety questions and that number was 69 people fewer than the entire sample shown here. Likewise and as stated in the paper, surveys were left with participants to complete, and then retrieved 30 minutes later; hence, we do not know how long it took participants to complete the survey -- it could have been 15, 20, 25, or 30 minutes. On page 7 of the revision, we've changed our estimate to 15-30 minutes to address this concern.

4. Ms. Yerger stated that the terms "non-smokers" and "cigarette non-smokers" are confusing. Non-smokers and cigarette non-smokers are people who don't smoke cigarettes; we are unsure of how to clarify this further and welcome suggestions. She also asked us to clarify the difference between a polytobacco-user and a multiple-product user. On page 5 of the original paper (in the paragraph before Methods), we defined smoking of other products by cigarette smokers as polytobacco use, and by non-smokers as multiple-product smoking. We believe that this is clear, but welcome suggestions about how to make this clearer.

5. Ms. Yerger stated that our data are consistent with the CDC's 2012 data on youth and that the CDC 2012 paper therefore should be cited. As requested, we have added this reference as #35 in the revision, and mention the study on page 13 of the revision.

6. Ms. Yerger stated that the implications for menthol cigarette smoking should be discussed. However, menthol-smoking was included in ALL regressions, and found to play almost no role. In the revision, we added a sentence to page 12 to underscore that menthol-smoking generally was not relevant to any of the findings. Please explain what there is to discuss about it.

7. Finally, Ms. Yerger stated that flavored cigarettes have been banned, and that we should discuss

the relevance of that ban to the findings. Our data were collected 2006-2008 (before the 2009 ban), and hence the ban has no implications for our findings -- except that use of banned products may have changed since our study (post-ban). In the revision, we discuss that on page 13.

MR. ALLEM

1. Mr. Allem noted that we treated the demographic variables as categorical instead of continuous. We agree that alternative analyses of these (and indeed, of any) data certainly are possible. However, we do not believe that treating the variables as we did renders our analyses inadequate or inappropriate, and we note that categorical variables are the norm in epidemiologic studies. To satisfy this concern however, we added the use of categorical variables as a limitation of the study (page 13). Likewise, Mr. Allem stated that analyses of interaction effects would be interesting. Although we agree, we note that to include potentially interesting interactions such as gender X age, gender X education, gender X income, income X education, gender X menthol/non-menthol etc. would increase the number of significance tests and the size of the tables. Moreover of course, not every, potentially interesting analysis can be included in a single paper, and generally, basic epidemiological studies of product-use do not include any interaction effects (e.g., the CDC study highlighted above included no interaction effects). To address this concern, we have added to the Discussion (page 13) that the absence of such analyses is a limitation of this study and of similar studies.

2. Mr. Allem stated that we should display the 95% CI s in parentheses. The 95% CI s were included in each regression table. We are unclear about why these should be repeated in the text as well.

3. Mr. Allem stated that the tables could be consolidated. Tables 3,4, and 5 however present not one but TWO DIFFERENT regressions to reduce the number of those tables to 3 (instead of 6). We are unclear about how to further reduce the tables and welcome suggestions.

4. Mr. Allem noted that other studies of smoked products generally do not include marijuana smoking and stated that we should justify including it. We included smoking of marijuana, cigarillos, bidis and other products that are not included in prior studies of adults in order to explore their use, and we explored their use because they may be relevant to unexplained smoking-related disparities among Black adults. In the revision, we've clarified this on pages 6-7. We believe that our data on past 30-day use of products not covered in other studies on adults adds to the literature precisely because we included products that others do not.

5. Mr. Allem stated that flavored cigarettes have been banned and that we should address how participants acquired these. As noted in response 7 to Ms. Yerger (above), the revision now addresses this issue.

6. Mr. Allem stated that it would be beneficial to add information on frequency of use of the products. Although we agree that such data would be interesting, we note again that everything interesting simply cannot be reported in a single paper. Adding data on frequency of use of each product would require many more analyses and tables (e.g., frequency of use by the demographic variables) -- and Mr. Allem believes that we already have too many tables. We believe that our data on past 30-day use of products not covered in other studies of adults adds something new and valuable to the literature even without analyses of frequency of product-use; likewise, we note that frequency generally is not included in basic epidemiologic studies (e.g., the CDC study). In the revision, we have noted that the absence of analyses of frequency of product use is a limitation of this study and of similar studies.

Mr. Allem also stated that we should discuss the role of tobacco taxes and prices in increases in use of the products. Given that this is a cross-sectional study, we have no data indicating that use of the

products has increased over time among our sample, and, there are no prior, comparable, statewide, California data on adults to use as a longitudinal comparison. Hence, we have no evidence that use of the products has increased among California Black adults. Thus, speculations about the possible role of policies, prices, and taxes in increases that we do not know exist seem to us be inappropriate. We hope that Mr. Allem agrees.

7. Mr. Allem asked if the frequency of cigarette use is related to use of the other products. As noted in above, answering this question would require many additional analyses and tables -- and Mr. Allem believes (as do we) that we already have enough tables. Moreover, almost all of these smokers, like Black smokers in other studies, smoked 10 or fewer cigarettes per day, leaving little variance for such analyses. We have added the absence of such analyses to our study limitations (page 13) where we also noted the number of cigarettes smoked per day by this sample of Black smokers. We believe this addresses the concern.

8. Mr. Allem stated that our hypothesis is ambiguous. We have removed it from the body of the paper, but not from the Abstract.

9. Mr. Allem asked for the years of the study. We added this to the Methods, Results, and Discussion sections.

10. Finally, Mr. Allem asked us to rearrange the Discussion so that it ends on implications and future research instead of on study limitations. We have done so.

We made many additional, minor changes in wording in the revision to make the revision consistent with the Reviewers' concerns. These minor changes, like the more major changes discussed above, are all shown in RED throughout the marked copy of the revised manuscript.

VERSION 2 – REVIEW

REVIEWER	Allem, Jon-Patrick University of Southern California, Preventive Medicine
REVIEW RETURNED	23-Aug-2013

THE STUDY	My initial concerns regarding the treatment of age and income still remain. The problem does not necessarily lie with using categories for age or income but rather the lack of justification for how these categories were selected. I agree with the authors that it's common in epidemiology to categorize a continuous measure but some explanation is generally provided e.g., categories were defined by deciles, quartiles, smoothing functions, etc. Certain estimates found in the results section provide further reason to refine age. For example, the estimated odds of smoking a Philly/Black & Mild cigarillo are 15.9 (95% CI, 7.48 to 33.81) times higher among 18 to 24 years olds compared to those 45 years of age or older. This estimate is very inefficient. I also still believe interactions should be explored and would find it beneficial if the authors demonstrated whether or not frequency of cigarette use is related to use of the other products. However, if the authors, other reviewer and editorial board believe these suggestions are more appropriate for a separate paper, I will acquiesce.

VERSION 2 – AUTHOR RESPONSE

First, Mr. Allem's statement that the age and income categories in prior studies were defined using criteria such as deciles, quartiles etc. is frankly incorrect. The age categories in prior studies of polytobacco-use and smoking were exactly the same or nearly the same as those we used, and the income categories were arbitrary. Three examples should suffice to demonstrate this point:

In our cited reference #13 (Delnevo et al., Prev. Med. 2004; 39: 207-211), age was reported as 18-24, 25-34, 35-44 etc. – precisely the same categories that we used. That study also used the same education categories that we used, and ignored income altogether. Likewise, our cited reference #3 was a recent, nationwide, population-based study of more than 100,000 adults (King et al. Am J Public Health. 2012; Vol. 102). In that study, participants were grouped as ages 18-24, 25-44, 45-64 etc. – categories very similar to ours. Income was treated as the arbitrary categories of <$20,000, $20,000-$49,999, $50,000-$99,999, and $100,000 and more. In another, very well-known, nationwide, population-based study of tobacco use, Lawrence, Fagan et al (Nicotine & Tobacco Research, 2007; 9: 687-697) at NCI focused on 18-24 year olds only – an age group that is often differentiated from other age groups in smoking and polytobacco-use studies. Those NCI researchers also used the truly arbitrary income categories of <$20,00 vs. $20,000 and higher in the study.

Clearly, in these 3 and in numerous similar studies, age and income were NOT treated as continuous variables, and deciles, quartiles etc. were NOT used to define categories as Mr. Allem claims. The methodological norm in this type of research is to use the age categories that we used (18-24, 25-34, 35-44 etc.), and to use 2-5 arbitrarily-defined income categories as well. Thus, our categorical variables are consistent with those used in this type of research, and do not require justification. Likewise, our analyses are consistent with published research on this topic and hence are not "inefficient." Indeed, by treating our variables in the manner that prior studies have treated those same variables, we assure that our results can be compared to results in the literature – something every journal desires. We note also that other reviewers had no concerns about our use of categorical variables.

Secondly, where interaction effects are concerned, we again note that studies of smoking and of polytobacco use almost NEVER include interaction effects. Hence here too our analyses are consistent with published research. THUS: We again decline to change age and income to continuous variables, and decline to conduct analyses of interactions, and we firmly believe that this decision is empirically justified. We also emphasize that reviewers request additional or alternative analyses ONLY when those conducted are incorrect or deviate from the literature -- not when they deviate from the reviewer's personal preferences. Our analyses and our categorical variables do not deviate from published studies, are correct, and were acceptable to the other reviewers.

That said, we want to note that in his prior review, Mr. Allem asked us to better explain/justify using blunts and marijuana as outcomes in our study given that these are not included in most studies of smoking or polytobacco use (i.e., our study deviates a bit from research norms). This was an entirely appropriate request to which we failed to respond. Thus, the attached revised manuscript includes the following changes:

On pages 4-5, we added a rationale for including blunts and marijuana in our study, and added an associated reference (#37). We believe that doing so strengthens the paper, and we thank Mr. Allem for this suggestion.

On page 12, we noted that our findings for 18-24 year olds match those of prior studies for that age group, and we cited those studies.

On page 13, we noted that the age categories we used are similar to those used in other studies.